# Rall, David P. 1997

## Dr. David P. Rall Oral History 1997

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National Cancer Institute Oral History Project

Interview with David P. Rall, M.D. at home in Washington, D.C.

December 30, 1997

Interviewer: Gretchen A. Case

**GC:** This is Gretchen Case of History Associates. This is the National Cancer Institute Oral History Project, and I'm speaking with Dr. David Rall at his home in Washington, D.C. Today is December 30, 1997, about four o'clock.

**DR:** I showed up at NCI [National Cancer Institute] at NIH [National Institutes of Health], on October 1st of 1953. NIH that year had a magnificent budget of $50 million. They got a few extra hundred thousand, so they hired me and hired one or two other people.

NIH was really very small. I worked in a little building called Building 8, which, if you stand on the front steps of Building 1, it's sort of off to your right and behind you. During the modernizing phase, Building 8, they said they simply remodeled it, but they tripled it in size before modernizing it. It was a very simple building then.

We almost kept animals into our own laboratories. We didn't have the big animal rooms which you're supposed to have. On the other hand, it was very convenient. One of the nice things was that everybody ate in the Building 1 cafeteria, because that was the only cafeteria, and you really got to know a large percentage of the scientists at not only NCI, but the other Institutes.

Building 1 also had the dentist, which [U.S.] Public Health Service officers could go to, and a number of other conveniences like that. Slowly we got moved out, and finally they didn't give free dental to Public Health Service officers anymore. But the Building 1 cafeteria was delightful. Everybody kind of had breakfast there, and lunch, you really saw almost anybody you wanted to.

We moved into Building 10 about a year and a half later. My group had one of the wings, 6 East, and we moved in and there was no furniture at all. I remember sitting on the floor of what would be my office when my lab chief and Dr. G. B. Mider walked by.

**GC:** So you were working, sitting on the floor?

**DR:** Reading. That's all I could do. Actually, when [C. Gordon] Zubrod came, I thought things got very much better. So one of my bosses was sort of strange, more interested in office politics than science. But we had freedom to do what we wanted. I was in a good laboratory, with some very good people in it.

But when Zubrod came, he really gave us a focus to the whole effort. The original focus was really acute leukemia in kids, and it was successful. I mean, we went from 0 percent cures and short remissions, few weeks, to, I guess now, around 70 percent cures. Now, what's going to happen to these kids later on, and it's getting later on now, I think some of them will come back with second cancers from the chemotherapy. But still, that's a major improvement.

Zubrod got us interested in one of the main problems, which blocked the further cure of acute leukemia, which was so-called meningeal leukemia, where the leukemia would get into the meninges and on the surface of the brain, and apparently they [leukemia cells] can live there during intense chemotherapy, because none of the drugs that were commonly used were able to penetrate the blood-brain barrier and get it under the meninges. So first we did a lot of studies about the normal blood-brain barrier, mostly in dogs, showing how lipid soluble compounds moved in very rapidly, and ionized, not lipid soluble compounds, moved in very, very slowly.

Then we found a very interesting effect that because, and I don't remember which, either the cerebrospinal fluid or the blood, is slightly more alkaline than the other. You'd get a partitioning of partially ionized compounds where the concentration would be slightly higher in one than in the other, which was interesting, but probably not important.

What we did was we also looked very extensively at other species than just the dog and cat and so forth. In fact, the director of the clinical center at the time—they named the main hall after him—objected, because we wanted to bring some, I think, sheep in or something like that. But we did have crocodiles and a bunch of other things.

**GC:** In the labs?

**DR:** Crocodiles this big.

**GC:** That's only about a foot long.

**DR:** Yes, they were not anything that you want to worry about. One of the most amusing things was once I got paged so that I could draw blood from a crocodile, which I knew how to do, and not many people did. [Laughter]

**GC:** How do you draw blood from a crocodile?

**DR:** They have a nice big artery and vein running down on the inside, behind the cloaca, down their tail you can usually get a needle and turn them over, upside down, and put a needle right in the middle, push it down to the spine, and you get blood almost every time, if you ever have to do it. Works very nicely with most fish, too.

**GC:** That's quite a skill.

**DR:** Yes, as a matter of fact, I was at a marine lab outside of Honolulu, and the guys there didn't know how to do that. They were pleased and surprised.

**DR:** Where was I?

**GC:** You were just talking about drawing blood from a crocodile and that you got paged . . .

**DR:** We did look at the various vertebrates to see where the blood-brain barrier began. It really began somewhere between the teliost, between the elasmobranch and the teliost. Sharks have a very weak blood-brain barrier and teliost—bony fish have a pretty good blood-brain barrier. If you go below the elasmobranches to the cyclostomes, there's absolutely no barrier at all. The cyclostome is largely the slime eel, which lives on the bottom of Frenchman's Bay. We worked mostly at the Mount Desert Island Biological Laboratory in Salisbury Cove, Maine. All you had to do was put—well, it's kind of like a crab trap down on the surface of the bay and bait it, and they'd crawl in, and then you could work on them. Awful animal.

**GC:** Really?

**DR:** Well, they squirted slime from their skins all over you while you were working.

**GC:** Hence, the name. Right?

**DR:** But we did, and we really got a nice picture of how all the animals' blood-brain barrier works. And in terms of the problem of trying to do something about meningeal leukemia in kids, there was a great controversy at that time as to whether or not there was any extracellular space in the brain. One picture which you saw in electronmicroscopy was the brain cells packed so tightly against one another, there was no space between them. Now, in almost every other tissue, the cells have space between them, with extracellular fluid. On the other hand, it was known that when you kill the brain, the cells swell. So this could well have been just a post-death artifact.

Well, Zubrod and I wrote a review for the *Annual Review of Pharmacology* on drugs getting in and out of the brain, and in looking at all of the literature for that, an idea occurred to me as to how you could test for the presence of extracellular fluid, and that was to perfuse the ventricles of the brain with a solution containing inulin, which is simply a moderate-size inert molecule, which was radioisotopically labeled, and you could see if it diffused into the extracellular space or not, and it turned out it did. It diffused in exactly the way physical chemistry would predict. There was a very nice 10 percent, 12 percent extracellular space. And we did the critical experiment, that is, we killed the dog and then perfused it, and there was no extracellular space.

Now, the reason that's important is that if the drugs couldn't get in the brain through the blood-brain barrier, perfusing it in the ventricles wouldn't do any good if the cells were so packed together. But if they were surrounded by extra cellular fluid, then the anticancer drug could diffuse into the brain itself, invade all the cells, and presumably also into the meningeal leukemia and cure the kid. And that's actually what happened. It turned out, however, that a low dose of X-ray was much easier to do, and about as effective.

The first time we tried it in a child with leukemia was something I'll never forget, because it was the afternoon [John F.] Kennedy was shot, and we were just ready to puncture this poor little kid's brains with needles on each side when we got word that he was shot, and I have never seen a place go so quiet or just fall apart. Nobody did anything. It was very strange and very sad, too. But later on we did it. We perfused very successfully. We showed it was therapeutically effective. It let us do interesting things like measure the rate of production of cerebrospinal fluid in people, live and so forth. So that was interesting science.

That whole part of those was interesting as to why NIH worked as well as it did. We were the Cancer Institute, and neurology people were very interested in this, but didn't really much care about leukemia. Then there were other people interested in various aspects of ion transport, and so forth. We all sort of spontaneously got together and led a series of informal seminars. This is from three or four Institutes that were just absolutely topnotch. I think it all helped us do a much better job at what we were supposed to do, and it illustrated why NIH was really such an effective place, where you had these different talents, and people able to look at a problem from a different point of view all around you. And, of course, Tom [Emil] Frei, and [Emil] J Freireich. Have you talked to them?

**GC:** Yes, I have. You must have known them pretty well.

**DR:** Oh, yes. Did Tom Frei give you his paper on Freireich?

**GC:** Yes. It's from the festschrift?

**DR:** Yes. It's wonderful. [Laughter]

**GC:** It is. It is wonderful. Tell me about working with them.

**DR:** Oh, it was great fun. I would echo the way Tom described it [in the festschrift paper]. J was absolutely infuriating about 5 percent of the time. Tom, on the other hand, tended to be patient. No, we had a good time. Again, this was very good collaboration between the basic scientists and the clinical scientists. We used to have seminars in what they used to call "Top Cottage."

**GC:** Top Cottage?

**DR:** Yes.

**GC:** That's the building?

**DR:** Yes. That's now where Building—what's the office building? I've forgotten the number of it.

**GC:** Building 6?

**DR:** No. Thirty-seven. No.

**GC:** Oh, 31. The big one?

**DR:** Yes.

**GC:** That's 31.

**DR:** Thirty-one. Yes. Top Cottage was a small house on the Wilson estate. It was just delightful for seminars. Mrs. Wilson was would occasionally wander around.

**GC:** Mrs. Luke Wilson?

**DR:** Yes.

**GC:** What was her name? I've always seen her referred to as Mrs. Luke . . .

**DR:** I don't know. And she would say hello to everybody and ask us what we were doing, just very nice and, I think, very, very proud of the fact of what they did, giving that [the land for the NIH campus] to the government, was, in fact, very, very useful.

**GC:** You're the first person I've met whose mentioned her. I've only seen her name in the documents turning it [the land] over to the NIH.

**DR:** Well, not many people around now were there in '53.

**GC:** What did she look like? I'm just a little bit curious about her. Do you remember much about her?

**DR:** I just remember her as small, and white hair, and pleasant.

**GC:** She would just periodically come and kind of check on . . .

**DR:** She used to go out for a walk and would run into various people since she was still living right in that area.

**GC:** That's wonderful.

**DR:** The other thing Mider used to do was he would prowl the corridors to find equipment out in the corridor which it wasn't supposed to be. Now, if you've been in Building 10 now, you know that equipment is all over the place.

**GC:** You told me right before we turned the tape recorder on a little story about Dr. Mider. Would you just tell me that again?

**DR:** Oh, that was just on a New Year's Eve afternoon. I was working away, not very enthusiastically, I must say, at that time, but he saw me, and we talked a little bit, and he said, "Oh, it's time to go home. It's time to go home. It's New Year's Eve." So we went home.

Then I had a wonderful time once. I had been told by the police that I couldn't use one of the exits which led directly to the place where I was parked, that I had to go around and use the main exit, and I obeyed. But two nights later, the cops were going out that exit, because their cars were [parked there]. So I wrote a very nasty memo asking if there was one set of rules for the scientists who actually did the work at the Institute, and another easier set of rules for the policemen.

**GC:** Did you get a response?

**DR:** Yes. I sent it to Jim [James] Shannon. They didn't do that anymore. [Laughter] There are really so many things. Well, maybe you—why don't you—

**GC:** I've got quite a few questions. You told me that you came to NCI in 1953.

**DR:** Yes.

**GC:** What exactly brought you there? Were you contacted, or did you apply to work there, or how did that happen?

**DR:** There was something called the Korean War. I had just finished my internship. I knew I didn't want to practice medicine; I wanted to do research. But I felt I needed a year at least of good clinical experience, so that I ended the internship on July 1st, and started looking around for whatever one of the armed or uniformed services was interested in me. NCI seemed very interested. It turns out October 1st, then, I joined as a senior assistant surgeon.

**GC:** Who hired you, do you remember?

**DR:** Murray Shear.

**GC:** He's someone I wanted to ask you about. His name comes up quite a bit. What was working with him like, or what was he like?

**DR:** He was okay. He was kind of strange. I think his scientific creativity had come and gone by the time he was lab chief. But there were good people in that laboratory of clinical pharmacology. Margaret Kelly. Gee, I can't even remember their names. In retrospect, one wishes one had kept a lot of the phone books.

**GC:** They have them. They have a full run of them. So when you first came here you said you wanted clinical experience. Did you go directly—

**DR:** I got clinical experience interning. No, I came here to do research, basic research. I did have clinical privileges, but I never used them. I did see an occasional patient and would occasionally make rounds, but what I wanted to do was the basic research.

**GC:** So you mentioned when you came that it was pretty easy to do the projects that you wanted to do.

**DR:** Yes.

**GC:** Did you come in on a project, or someone handed you a project? Or did you come in and say, "This is what I'm interested in"? How did that work at that time?

**DR:** I had gotten my Ph.D., and my thesis was on pyrogen-induced fever, bacterial pyrogen. I think one of the reasons they were interested was that there had been a guy in New York who thought that giving patients with cancer infections would cure their cancer, and he wasn't entirely wrong, because he did have some very good examples of cure, and now it's clear that when you get a bacterial endotoxin, which is what it really is, some of the lethal tumor necrosis factor is released. So there was really something to that. We didn't stumble on that, unfortunately. But we worked on a lot of aspects of the pharmacology of bacterial pyrogen.

Then, as I said, when Zubrod came, I became very much more interested in blood-brain barrier. Then I got interested in some aspects of—well, one of the problems was its relatively easy to kill cells which are rapidly dividing, because they're very sensitive during the time they synthesize their DNA and so forth. But it's very hard to kill cells which are just sitting there. So we were trying to devise ways of doing that, certainly using alkylating agents which tend to kill any cell it comes in contact with, whether it's dividing or not. We worked with Howard Skipper, whom I'm sure Zubrod talked about.

**GC:** Quite a bit.

**DR:** On how to develop a drug that would penetrate into the brain and cerebrospinal fluid. One other thing I did which really was kind of exciting and difficult, and you can learn more about it from Mike Walker, was we organized neurosurgeons to have a cooperative clinical group on brain tumor chemotherapy. And organizing neurosurgeons is a little like herding cats.

**GC:** In that they don't want to be organized?

**DR:** No. [Laughter] But it was fun. They got some good studies going. That happened just two or three years before I went down to the North Carolina. But that was very interesting.

**GC:** This is very basic, but I wanted to go back and just check. The name of your lab was the Lab of Chemical Pharmacology. Is that right?

**DR:** Yes.

**GC:** Was that a direct descendant of the Division of Pharmacology? When the NCI was founded, it was the NIH Division of Pharmacology, and then the Office of Cancer Investigations from Harvard. Those two were merged. Do you know if that was directly out of that division that had been based in—

**DR:** I think, yes, that Chemical Pharmacology came out of the group that was at Harvard, for some of them, not all of them. Do you have the names of the people who were in that, at Harvard?

**GC:** The people that I have that were original staff members that I was thinking would have gone into group were Murray Shear, who you mentioned.

**DR:** Yes.

**GC:** J. M. Venditti.

**DR:** Yes. Where did he come from? John. No. He came into the group, yes, but I can't remember where he started.

**GC:** And Abraham [Abe] Goldin.

**DR:** Yes, he was there. He was Venditti's boss.

**GC:** Did you work with those two, Venditti or Goldin?

**DR:** We were kind of rivals. [Laughter]

**GC:** Oh, really.

**DR:** Yes. We worked together, but there was a real rivalry.

**GC:** In terms of projects going on?

**DR:** Yes. No particular details, but we both wanted more space and more people.

**GC:** How were resources at that time? I know that was a time of great growth for the NCI, for the NIH.

**DR:** Yes. Well, laboratory space was always the thing you really needed, because at least then, getting money and getting personnel slots was not a big problem, but laboratory space was very precious.

**GC:** But in terms of getting equipment or getting the people you needed, that was—

**DR:** Well, we had plenty of money, but purchasing could be just awful. Yes, and the purchasing agents were always interested in the cheapest. One of the worst things was getting constant-temperature rooms, because there were a couple of companies that outbid everybody else, and then typically defaulted, and the people in purchasing wouldn't let us take those off the list. But this meant getting a constant-temperature room could take literally a couple of years, because finally they would see that, well, yes, this company always gives the lowest bid, but it never does anything.

This was also related to something we tried to fund. Margaret Kelly was one of the first to show that newborn baby animals were very much more sensitive to carcinogens than other animals. We thought this might be able to let us develop consistent tumors in monkeys, primates, which are fairly close to us, so that we could study chemotherapy in a primate rather than a rat or a mouse. So we arranged to set up a breeding colony of rhesus monkeys at Hazelton Laboratory. You can imagine the problems we had with it. Purchasing agents who really didn't understand that getting live, female, healthy monkeys from Africa, and so forth, was very much more complicated than buying a refrigerator. Just working with these guys, they were just stubborn. But we finally got a very, very good breeding colony that I think still exists. I know Dick Adamson was taking care of it two or three years ago.

**GC:** Was that the Hazelton that was down in Reston, Virginia?

**DR:** Yes. I think I started Hazelton in the monkey business.

**GC:** Really.

**DR:** Yes. Which got them into, ultimately, their fame as Ebola virus.

**GC:** Ebola Reston, yes.

**DR:** Yes. I think I started them. Of course, nobody had really done this with a colony of monkeys yet, so you didn't know really quite how you did it. It all worked very well, and we got some nice tumors. As I said, as late as a couple of years ago, they were still using them.

**GC:** That's amazing.

**DR:** When did I do that? When did we do that?

**GC:** Was that in the fifties or was that later?

**DR:** Yes, it was in the late fifties, I think. The other thing I did was testing anticancer drugs for toxicity. Not so much for therapeutic effect, but what are the toxic effects? What sort of dosage could you get away with? And so forth and so on. We did a lot of that, and I had a big contract with Hazelton to do much of the actual toxicity testing in their laboratory.

**GC:** This was while you were still at NCI, though?

**DR:** Yes. Oh, yes. This was, again, in the late fifties or early sixties.

**GC:** Oh, I see. You contracted out to them to do the work.

**DR:** Yes.

**GC:** I'm sorry, I was reversing it. Okay. The National Cancer Chemotherapy Program started in 1955.

**DR:** Yes.

**GC:** How did that fit in with what you were doing?

**DR:** We all worked together. They were the ones that obtained new chemicals and put them through routine screens to determine anticancer activity. If it looked very promising, then I'd begin to look at the first rodent, and then dog, and primate toxicity to see what sort of dosage you could use in people. We worked very closely together.

**GC:** Do you remember with whom you worked at that program? Were there people you worked with there very closely?

**DR:** Well, Venditti was one, and Abe Goldin.

**GC:** And when you were working with Dr. Frei and Dr. Freireich, how did that work? How did you work together as a team? Would you suggest ideas to each other? Would you have constant meetings?

**DR:** Yes.

**GC:** What kind of working pattern did you use?

**DR:** You ran into people all the time.

**GC:** Just in general?

**DR:** Yes, and we all socialized together, too. I remember Tom Frei wondering around with a great big butcher knife in his hand at one of our evening parties. [Laughter]

**GC:** Oh, really?

**DR:** Very impressive.

**GC:** Yes, I've heard a few party stories. You mentioned Margaret Kelly. Was there anyone you worked with closely in your lab?

**DR:** There was Ti Li Loo.

**GC:** He's still in town, is that right?

**DR:** No, he went to Houston, Texas. Then a number of nice clinical associates came through.

**GC:** Anyone in particular?

**DR:** No, not really. If I saw a list I could probably—but some were very good.

**GC:** Did you have any contact with the directors of NCI, or the deputy directors, or any of the higher administrators?

**DR:** Yes. Not at first, but once we moved into Building 10 and so forth, and I became an associate scientific director, which I think we met every week or every other week.

**GC:** You came in under [Rod] Heller, but you probably didn't have much contact with Heller.

**DR:** Yes. Actually, Heller, everybody did have a lot of contact with him. He walked around and he talked to people, and so forth.

**GC:** What was he like?

**DR:** He was very nice. One didn't get a sense of how good a scientist he was like Mider, but he was extremely affable and an extraordinarily nice person.

**GC:** So he would come and check on the labs?

**DR:** Yes. Or just wander around to say hello.

**GC:** That's good. Then Kenneth Endicott came after him. Did you know him very much?

**DR:** Yes. Oh, yes, I knew him very well.

**GC:** Did you? And what was he like, or what was it like working with him?

**DR:** A superb politician and he loved being a politician. He understood how organizations worked. The next one was—

**GC:** The next one would have been Carl Baker.

**DR:** Carl's still around.

**GC:** Yes, I've spoken to him.

**DR:** You talked to him. Yes, Carl and I were always going to Russia. That was another thing we did.

**GC:** So you went on some of those trips with him?

**DR:** Yes. And with Roger Egeberg, who just died four or five months ago.

**GC:** Tell me about the trips to Russia. What was your role?

**DR:** I had one of the series of projects with toxicologists in Russia. Moscow is really mostly where they were, Kiev to some extent. Well, the Russians were the Russians in those days. You had an enormous amount of vodka, which you almost couldn't get out of drinking.

Then we devised the Bacon Index. When shopping in Moscow, you had to find out how much something was, and if it was available, you had to wait in line for that. Then you had to wait in line to pay a clerk, who would then give you the proper slip of paper to take back to get the item which you wanted. It took a lot of time, but when we first started going in about '73, bacon we would have called very fatty, but clearly there were some strips of meat in the bacon. By the time we stopped, about '78 or '79, you couldn't find any of the meat anymore. We think this predicted the essential downfall of the Russian enterprise. The CIA never accepted this as a legitimate index.

**GC:** Did you present it to them?

**DR:** Yes. When they were going to interview us, I'd keep asking them questions, could they find out something for us, and we never got anything out from them. [Laughter]

**GC:** So they would debrief you when you came back?

**DR:** Yes.

**GC:** Well, good for you.

**DR:** The funny thing was, you could always tell who the KGB guy was. He tended not to have any technical expertise. He also was the one who was comfortable brokering compromises. They tended to be problem-solvers rather than some of the scientists were incredible egos, very hard to deal with. But KGB could usually make the agreement come out.

**GC:** So they were useful, right?

**DR:** Some of the Russians were delightful. We became very good friends with some of them. It was, well, it was very exciting to go to Russia and wander around there.

**GC:** I'm sure, especially at that time.

**DR:** At that time, yes.

**GC:** You were probably one of the few people who really got to go over there.

**DR:** Apparently, it was [Secretary of State] Henry Kissinger's idea that if there were a lot of Americans wandering around Moscow, and a lot of Russians wandering around the United States, it would be a little harder for either country to send a rocket to the other country to blow it up. I don't know. That's what people in the White House said about the détente. Some good work was done, not a lot.

**GC:** So you worked with Dr. Baker pretty closely then on these Russian trips.

**DR:** Yes.

**GC:** And then after that was Dr. [Frank] Rauscher.

**DR:** Dick Rauscher, yes.

**GC:** Did you know him?

**DR:** Oh, yes. He and I were sort of contemporaries. He was a virologist, so we didn't interact that closely, but we knew each other. We were really very good friends.

**GC:** And he's passed away.

**DR:** Yes.

**GC:** Then Dr. [Arthur] Upton.

**DR:** Yes, I knew Art very, very well. He was very helpful. Of course, NIEHS [National Institute of Environmental Health Sciences] was just a struggling institute at that time.

**GC:** Then did you work with Dr. [Vincent T.] DeVita?

**DR:** I hired Dr. DeVita.

**GC:** Did you?

**DR:** Yes.

**GC:** Tell me about that.

**DR:** We would hire these bright young clinical associates, and he looked very good. We liked him for a couple of years, and after sent him up to Yale for, I guess, a year or two, to get some more clinical experience. He ran the clinical service when he got back. We were together on that big paper on extrapolation. I have a reprint of it upstairs—what's the toxic dose in mouse, rat and human? For many anticancer drugs, the relationship was pretty good.

**GC:** I think I might have that one, too.

**DR:** Yes, that's one of the most cited papers, and J Freireich was the senior author because we did it alphabetically.

**GC:** Oh, really?

**DR:** Yes. Actually, the Southern Research people, Skipper and I, did most of the work, because I got all the mouse and rat stuff, data at Hazelton. That was a good paper.

**GC:** I noticed on your résumé, or on the information I have, that you were a trustee of Mount Desert Island.

**DR:** I was, I'm not anymore.

**GC:** Can you just tell me about the relationship between NCI and the lab up there? It comes up all the time.

**DR:** Really?

**GC:** Yes.

**DR:** Gordon Zubrod had been up there with Shannon three or four years before he came to NCI. He invited me to go along, and I fell in love with it, and my family totally fell in love with it. We worked on various animals. I found that the choroid plexus of the dogfish can secrete organic acids, which is an interesting finding. It acted a little like a kidney. Mostly it was the blood-brain barrier and that sort of stuff that we did.

**GC:** And you were working on that up there?

**DR:** Yes. I almost got in trouble with it. The director of one of the other Institutes would go up to Woods Hole [Oceanographic Institute] and spend the whole summer, and when that leaked out, everybody got furious. I was not a full director, I was a lower-level assistant. I never did that. I spent a lot of time up there, but I would come back every couple of weeks for a couple or three days, and he took per diem the whole time, even though he owned his house up there. I finally did own a house up there, and what I did was take per diem for a couple of days going up and a couple of days coming back, and nothing else. So when that all came out, I looked very good.

**GC:** I bet. So he treated the whole summer as kind of a business trip.

**DR:** Yes. He did good research there was never any question about that. But as an Institute director, it really wasn't very good for him to desert his Institute, and I gather he really didn't call back and talk to them about what was going on.

**GC:** One question I've talked to a lot of people about, and I don't know what your experience with this was, is bringing in contracts. You mentioned contracting with Hazelton. There seems to have been kind of a controversy between grants and contracts. What was your experience with that, or how did you feel about it?

**DR:** I thought both were very useful. They're just different, that's all. The grants people thought that it was not very good science [using contractors]. Sometimes it wasn't. But my sense was there are an awful lot of simple questions which just had to be answered. We needed the answer. Even if it wasn't great science, it was worthwhile doing. Yes, there was one of the outside reports that thought that was bad, too. I can't remember.

**GC:** That contracting was bad.

**DR:** Yes. Oh, yes, I remember it well.

**GC:** It was a pretty ongoing controversy, wasn't it?

**DR:** Yes. I think it still is.

**GC:** Yes, I think that, too.

**DR:** Well, when I got down to NIEHS and finally took over what was the NCI bioassay program, that's essentially all contracts, and always was. One of the troubles was NCI hadn't monitored the contract closely, so there were some bad ones which we—we got investigated by GAO [Government Accounting Office], and then [Senator John] Dingell [Jr.] asked them to come back and see whether we could fix the problem. By gosh, we got a GAO report that said, "Yep, they fixed it. Looks okay now." Which was—"Thank God!" That Dingell hearing was wild, because our people, who we had inherited, had just not followed these contracts very carefully. Clearly you had to monitor; you have to keep them on their toes. So it all worked out.

**GC:** When the National Cancer Act was passed in '71, where were you and how did that affect you? Were you involved in it at all?

**DR:** Was it in '71 or '70?

**GC:** Well, it was in '71.

**DR:** Well, that's interesting, because I left in '71. I guess so. I'm trying to think back whether—I think there'd been some increased appropriations before that, with the idea of an effort, and, yes, I was on my way out when they devised a Great Cancer Plan and so forth. And the great charts.

**GC:** The round charts?

**DR:** Yes. I actually found those pretty good once we got into it.

**GC:** They're pretty interesting.

**DR:** Yes.

**GC:** Were you involved in putting those charts together?

**DR:** Not very much, because, as I said, I was just moving out. I did not get involved in that. We did some of that for the chemotherapy program.

**GC:** The planning?

**DR:** That sort of planning beforehand. Because much of the chemotherapy program was kind of analogous to a mechanical factory process. You had to do this and this and this and this, and if this happened then that happened. Actually, it became sort of fun to see the development of the drugs, but I wasn't really involved with the big cancer planning.

**GC:** When it [the Act] did pass, went through and everything, did you have any thoughts about how this was going to change the NCI, or did it really affect you?

**DR:** No, because I left the NCI essentially by then and was down in North Carolina.

**GC:** So you were right outside Raleigh then [in Research Triangle Park].

**DR:** Yes.

**GC:** How long were you down there?

**DR:** From '71 to '90.

**GC:** Do you miss it?

**DR:** I miss what Chapel Hill was when I was first there. It was charming. And it's still charming, but not as much.

[Brief Conversation about Chapel Hill]

**GC:** Well, getting back to this a little bit. Were you ever involved in the viral oncology, the viruses-cancer program, or did you know anything about that?

**DR:** I didn't. I wasn't involved. We all met and talked about it, but I was never an expert, or all that much interested in it, I must say.

**GC:** That was just another ongoing controversy that I try to ask people about to see their opinion. What was the Clinical Center like when you came to NCI? It had just opened the year you came, is that right?

**DR:** Yes.

**GC:** In '53.

**DR:** Yes, it was very exciting. It really was the first, I think, the first time that basic science and clinical medicine were really absolutely together. The first clinical associates we got in those days, they are now the leaders in American medicine. It's incredible. We were there in the late fifties and early sixties. It was just an exciting place to be. You heard about the newest medicine, and everybody tended to go to Grand Rounds. There was just a great feeling of excitement.

**GC:** So you went to Grand Rounds.

**DR:** Yes.

**GC:** You said once in a while you saw patients. Did you follow up on people that you'd been working with?

**DR:** Yes, mostly it had something to do with something we were working on. Sometimes we would start a new drug and I would work very closely with whatever clinician was doing it. We had one of those incredible occurrences where we were going to start a new drug which was supposedly anti-radiation sickness, and it was ten o'clock in the morning we were going to get the drug and follow this patient. Something happened and we couldn't do it, and the patient died at eleven o'clock.

**GC:** Oh, my gosh.

**DR:** I mean, she was terminal cancer, we knew that, but what it would have meant if we had given the drug and the patient died an hour later! It turned out not to be a very useful drug at all.

**GC:** You mentioned that there was a lot of contact between the Institutes and a lot with the neurology people. Were there other Institutes that you worked very closely with?

**DR:** In one very interesting way we worked with the Dental Institute. There was an interesting story behind that. Somebody made the observation that at autopsy, one patient's tumor fluoresced brilliant yellow, and nobody could figure out exactly why. I got involved in that. I read the patient's chart, which people tend not to do. The chart has two things its got orders, and then it has what the nurses actually do. I also heard on a site visit that the tetracyclines were a very fluorescent color. Tetracycline was never ordered in the orders, but in the nurses' notes, they talked about putting up an IV with tetracycline in it. Sure enough, if you give tetracycline to a mouse or rat with a solid tumor, the tumor fluoresces intensely. It goes into teeth, also. So I did work with the Dental Institute for a while. It didn't really come to much. It looked this might be a way of getting drugs into the tumors, but it never came down to—but it was interesting stuff, because the localization of the tumor was very clear, very spectacular.

**GC:** So how did you make the move to NIEHS? What was the connection there?

**DR:** Bo Mider.

**GC:** He keeps coming back.

**DR:** He was chairman of the selection committee for the director of NIEHS. I don't know whether I would have even been interested had something else happened along the way. Jesse Steinfeld, who also is dead and was a good friend of mine, had been down in the assistant secretary's office. He was finally assistant secretary. He had asked me to—well, Nixon had started a look into prescription drugs, whether they were safe and so on. That investigation had finished, and that was by the guy who was still there but just stepped down as Assistant Secretary for Health, Phil Lee. Phil Lee's group also recommended that the FDA [Federal Drug Administration] be looked at as to how to make it more efficient and so forth. So Jesse thought I ought to chair a little intra-HHS-HEW committee to look at FDA. So I sort of took off four or five months and spent most of my time working on that, and delivered a document to Roger Egeberg on how to improve the function of the FDA. But what that did was give me some experience on Capitol Hill in the higher reaches of the department. I found it fascinating, so that when then I had a chance to head my own little Institute all by myself [NIEHS], even though it meant moving from Bethesda [Maryland], where I was very happy, I went and enjoyed that very much, too.

**GC:** Did you get called to the White House ever or have contact with the—

**DR:** Not too much, once or twice.

**GC:** What was that like? What kind of business was that?

**DR:** Oh, it was mostly some technical matter about pollution or something.

**GC:** Must have been pretty interesting, though.

**DR:** Yes.

**GC:** You said you enjoyed your work when you were kind of on the Capitol Hill side of things.

**DR:** Yes. I enjoyed that very much. I got to know some of the congressmen. And one, Dave [David R.] Obey, is still a very good friend. He was just starting out as a congressman when I was just starting out as an Institute director. I was telling about how terrible asbestos was, and we were the last Institute up when the committee adjourned, and Obey sat across the table and said, "You know, I worked my way through high school, college, and graduate school, selling vinyl asbestos tile." So we talked about asbestos for a while. He's been the Institute's best supporter.

**GC:** Did you know Michael Shimkin at all?

**DR:** Yes, not very well. I really didn't know him very well. I admired him very much. A marvelous guy, and did some superb research.

**GC:** He's one of the people who's actually written a bit of the history of the Institute. It's a very personal history, but kind of interesting.

**DR:** Have you talked to Harold Stewart?

**GC:** He declined.

**DR:** That's too bad.

**GC:** It is too bad. I would really like to talk to him.

**DR:** Did he sound okay? Healthy?

**GC:** He sounded very healthy.

**DR:** Because I talked to him about three, four years ago, and he was sharp as a tack.

**GC:** From all accounts I've heard, he's absolutely completely healthy, and he just declined. It breaks my heart; I would love to talk to him, but he's not willing to talk right now.

**DR:** I hired a pathologist and that drove him up the wall.

**GC:** Because you hired a pathologist?

**DR:** Yes.

**GC:** Because that was his—

**DR:** He was supposed to have the only pathologist.

**GC:** Oh, really. How did that all work out?

**DR:** I kept my pathologist. It probably gave Gordon [Zubrod] some grey hairs.

**GC:** So did you work with Dr. Stewart directly?

**DR:** Yes, we actually did work together pretty well. Not all that directly, but some. I know with the tetracycline stuff we worked together.

**GC:** If you ever talk to him and want to convince him to interview, that would be wonderful. I think he's the only one who's been there since the Institute started.

**DR:** Yes, he was one of the people at Harvard.

**GC:** He came down from Harvard and he's been there all the way through, and I understand he still goes into the lab every day and just checks things out. I would love to talk to him, but he just wasn't interested right now.

So when you left in 1990, you left the NIEHS. That was to retire, is that correct?

**DR:** Yes.

**GC:** Do you still keep in contact with the NCI or with people at the NCI?

**DR:** Oh, yes. I called one of them last week.

**GC:** Who do you still keep in contact with there?

**DR:** That was Victor Fung.

**GC:** I don't know him.

**DR:** He actually worked for NIEHS for a number of years. He had some information I wanted, and I knew he had it.

**GC:** Do you still do research, or do you still keep active at all?

**DR:** I keep active, but not doing research. I'm on the United Autoworkers, General Motors, occupational health advisory board, which is kind of fun. We get to visit automobile plants, and that's interesting. And I worked with the Environmental Defense Fund quite a bit, and Physicians for Social Responsibility.

**GC:** That's a really interesting group.

**DR:** Quite a lot, too. I keep in that one. I give an occasional talk. I talked two or three weeks ago at a course in the NIH night school.

**GC:** What would you say in your time at NCI you are most proud of , or most excited about, or what was your favorite thing about doing research or working there? What stands out to you, or does anything stand out to you?

**DR:** Oh, what really stands out is figuring out how to measure the extracellular space in the brain and doing it successfully. We started a whole round of research.

**GC:** That is quite amazing. Were there any pitfalls of working at the NCI? Were there any things that made research difficult or anything that was negative about working there?

**DR:** Not really. I think it was an almost perfect place to do research.

**GC:** Because of the freedom?

**DR:** The freedom and the support.

**GC:** And that sense of community that you talked about?

**DR:** Yes. Well, research is always fun. It's asking interesting questions and getting interesting answers. But it was a very happy time, I think, for everybody.

**GC:** You mentioned a few names, but is there anyone you haven't mentioned that you think I should speak with? I was planning on contacting Ti Li Loo at some point.

**DR:** I don't think so. Sounds to me like you talked to most of the people. Who else have you talked to?

**GC:** I've talked to Dr. Frei and Dr. Freireich. I've talked to Dr. DeVita, Dr. [Alan] Rabson, Dr. Upton, Dr. Baker, Bayard Morrison.

**DR:** Yes. Yes.

**GC:** Did you know him?

**DR:** Oh, sure. Sure.

**GC:** And I'm talking to Calvin [B.] Baldwin [Jr.] tomorrow.

**DR:** That's a very good source.

**GC:** Did you know Mr. Baldwin?

**DR:** Oh, yes. I think he's the best administrative agent there ever was.

**GC:** That's what I've heard. And he was there during the whole National Cancer Act passage, too, so I'm anxious to talk to him.

**DR:** Sounds like you've talked to the right people.

**GC:** I've got a list of quite a few more, too, that I'm planning on talking to.

**DR:** Well, if I think of other people you ought to talk to, I'll give you a call.

**GC:** Okay. Is there anything that I haven't asked or I haven't covered that you think you'd like to add?

**DR:** I don't think so. I'm just trying to sort of think if there's any—

**GC:** I know something I wanted to get back to. You said you came in during the Korean War. Was it called the "doctor draft" at that time? Or was it just during the Vietnam War that they called it the "doctor draft."

**DR:** Doctor draft, that's what they called it.

**GC:** Did they call it then, too?

**DR:** Yes.

**GC:** The other thing I wanted to ask you, what was a typical day like for you? Would you come in at eight, or nine, or seven, and what would you—

**DR:** Between eight and nine, and would usually end up about five-thirty. I found one thing that was terribly useful, and that is one day a week I'd just stay and work through, mostly reading and organizing 'til around nine-thirty, and that would let me keep my head above the mess. With free access to the library, open stacks, you could really get an enormous amount of literature work done there, and it was peaceful, nobody to talk to, so you worked.

**GC:** Did you ever have to do any late nights, or all weekend, or anything like that for a project?

**DR:** Occasionally when you wanted to do something, you wanted to follow animals around the clock, but not very much.

**GC:** So it was pretty much an eight-thirty to nine to five-thirty kind of day.

**DR:** Yes. And often, weekends and Sundays, but not always.

**GC:** Was that just checking in on things usually?

**DR:** Yes, or if you had a lot of work to do, or were still busy writing a paper, you might go back. I began to do that in North Carolina. What I would do would be to come in every Sunday morning and spend about three or four hours trying to get rid of all the paperwork from the past week.

**GC:** Get your desk cleared off, right?

**DR:** Yes.

**GC:** When you were working for NCI, did you live close by to the Institute?

**DR:** Yes, about three miles.

**GC:** Oh, that's nice.

**DR:** Yes, it was. Life was very easy and simple.

**GC:** And you said you really liked Bethesda a lot.

**DR:** Yes.

**GC:** Was that what brought you to this area originally, was to work for the NCI?

**DR:** Yes.

**GC:** Because you had been in Chicago.

**DR:** Yes, I grew up outside of Chicago. Then I went to Northwestern [University], which is in downtown Chicago, and I spent one year interning at Bellevue in New York.

**GC:** Wow that must have been interesting.

**DR:** It was fascinating. I saw absolutely everything.

**GC:** I'm sure. I'm sure.

**DR:** What was really incredible was the first month, where the first days you hadn't the faintest idea what was going on. It took about six weeks to really learn to be a doctor and feel that you could do everything, and then to be able to do it rapidly enough so that you weren't up all night. After about two or three months, although it was a terrible job in that you were up half of every other night at least, it really became fun. But you had to get used to it and get into the swing of it. I mean, every hospital's different in the way the hospital works.

**GC:** So that must have been quite a shift when you came to NCI.

**DR:** Oh, yes. Well, I'd done a lot of research at Northwestern. The easiest was the emergency room, because you worked an eight-hour shift.

**GC:** And then you went home.

**DR:** Yes. I'm not sure we worked eight hours; maybe we worked twelve hours. But we got real time off, whereas when we were on one of the services, you never really had time off. But I think I got a sense of medicine, which was really what I wanted.

**GC:** And that probably really informed your research later.

**DR:** Yes, yes, that's why. That's what I wanted to do.

**GC:** Well, do you feel I covered it?

**DR:** Yes, I think you probably covered it. I hope I'll get a copy of what you write.

**GC:** Oh, absolutely. Let me just stop the tape. I'm ending the interview with Dr. Rall, December 30, 1997.